A CRITICAL REVIEW OF THE 1986 SURGEON GENERAL'S REPORT, THE EPA RISK ASSESSMENT, THE NIOSH CURRENT INTELLIGENCE BULLETIN, AND OTHER RISK ASSESSMENTS ON ENVIRONMENTAL TOBACCO SMOKE (ETS)

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The conclusions of reports by three governmental agencies have been relied upon extensively by various organizations and individuals in discussions of health effects purportedly associated with environmental or "passive" tobacco smoke (ETS) exposure in the workplace. These reports are:

[1] The Health Consequences of Involuntary Smoking,[1] a 1986 report of the Surgeon General (hereinafter: 1986 Surgeon General's Report);

[2A] Health Effects of Passive Smoking: Assessment of Lung Cancer in Adults and Respiratory Disorders in Children (EPA/600/6-90/006A),[2A] a review draft released by the Environmental Protection Agency (EPA) in 1990 (1990 Draft Risk Assessment);

[2B] Respiratory Health Effects of Passive Smoking: Lung Cancer and Other Disorders (EPA/600/6-90/006B),[2B] a review draft released by the Environmental Protection Agency (EPA) in 1992 (1992 Draft Risk Assessment); and

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[2C] Respiratory Health Effects of Passive Smoking: Lung Cancer and Other Disorders (EPA/600/6-90/006F),[2C] a risk assessment released by the Environmental Protection Agency (EPA) on January 7, 1993 (Final Risk Assessment); and

[3] Environmental Tobacco Smoke in the Workplace: Lung Cancer and Other Health Effects,[3] a Current Intelligence Bulletin issued by the National Institute for Occupational Safety and Health in 1991 (NIOSH CIB).

These reports have been referenced by several respondents to the OSHA RFI in support of the regulation of smoking in the workplace. As discussed in the following comments, the reviews do not provide an acceptable scientific basis for regulation of smoking in the workplace.

1986 Surgeon General's Report

The EPA Risk Assessment and the NIOSH CIB rely on the conclusions of the 1986 Surgeon General's Report on "Involuntary Smoking," the U.S. Public Health Service's eighteenth report, and the fifth report issued during the tenure of C. Everett Koop. The 1986 Surgeon General's Report reached two major conclusions which some urge are relevant to the workplace smoking issue:

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> Involuntary smoking is a cause of disease, including lung cancer, in healthy nonsmokers. (p. vii)
>
> The simple separation of smokers and nonsmokers within the same air space may reduce, but does not eliminate, the exposure of nonsmokers to environmental tobacco smoke. (p. vii)

The Surgeon General's review has been challenged by a number of critics. One reviewer, Ann Fettner, suggested that the Surgeon General's conclusions were based on "flimsy" evidence presented in an effort to "divert our attention" from important health concerns such as the "poisoning of the environment."[4] A U.S. Congressman, Walter B. Jones, in a letter published in the Congressional Record, wrote that "the conclusions in the Surgeon General's Report are not supported by the research in his own report."[5] The Surgeon General's conclusions regarding lung cancer, cardiovascular disease, adult respiratory disease and the separation of smokers and nonsmokers are relevant to OSHA's current considerations of the workplace environment and are discussed below.

Lung Cancer

The Surgeon General's conclusion that a causal relationship exists between reported ETS exposure and lung cancer in nonsmokers was based on 13 epidemiologic studies of women whose husbands smoked. Of those studies, eleven reported overall risk

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estimates for spousal smoking that were not statistically significant.[6] Moreover, numerous scientific deficiencies in the epidemiologic studies on spousal smoking and nonsmoker lung cancer have been identified. A German specialist in biometrics and epidemiology, Karl Uberla, in his assessment of those studies, suggested that the data fail to meet the criteria necessary to permit a judgment on causality:[7]

> The majority of criteria for a causal connection are not fulfilled. There is no consistency, there is a weak association, there is no specificity, the dose-effect relation can be viewed controversially, bias and confounding are not adequately excluded, there is no intervention study, significance is only present under special conditions and the biologic plausibility can be judged controversially.

An American statistician, Nathan Mantel, observed:[8]

> [I]t is unlikely that any epidemiological study has been, or can be, conducted which could permit establishing that the risk of lung cancer has been raised by passive smoking. Whether or not the risk is raised remains to be taken as a matter of faith according to one's choice.

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There are currently 33 spousal smoking studies, 27 of which report overall risk estimates that are not statistically significant.[9-41] Only six studies report overall statistically significant increased lung cancer risks for women whose husbands smoke. The 33 studies, either taken as a group or considered

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individually, do not convincingly support rejection of the null hypothesis of no association between spousal smoking and lung cancer in nonsmokers, and therefore are inadequate as a basis for regulatory action.

The vast majority of the spousal smoking studies report risk estimates that are less than 2.0. (Eleven studies report cumulative estimates of risk of 1.0 or less than 1.0.) Risk estimates below 2.0 or 3.0 have been described as "weak," because such estimates are at the limits of detection for epidemiology.[42] This is particularly important because the spousal smoking studies themselves fail to account for numerous sources of potential bias and confounding factors. Confounding factors are variables associated with both the classification of "marriage to a smoker" and with lung cancer, the existence of which can give rise to a spurious ETS-lung cancer association. Examples of confounding factors that are not controlled for in most of the studies include diet, alcohol consumption, cooking and heating exposures, personal medical history, occupational and outdoor pollution exposures, and other lifestyle variables.[43]

The epidemiologic studies on lung cancer and respiratory disease cited in the Surgeon General's Report did not include any actual measurements of study subjects' exposure to ETS in either the home or the workplace. Instead, the studies relied on

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questionnaire information to estimate exposure to ETS. The Surgeon General's Report itself acknowledges that the "possibility of reporting bias must be considered for the studies that have used questionnaires to measure illness experience" (p. 38). A number of researchers have reported that reliance upon recall can lead to improper indices of exposure and incorrect estimations of risk.[10,44-49] The Surgeon General's Report also concedes that "validated questionnaires are needed for the assessment of recent and remote exposure to environmental tobacco smoke in the home, workplace, and other environments" (p. 107). The National Research Council and other authors have recently criticized questionnaires used in ETS studies for not being standardized or validated, pointing out that misclassification of exposure occurs when questionnaires are not appropriately designed.[50-52]

The application of spousal smoking studies to the workplace presents significant questions regarding extrapolation of risks and exposures. Even the Surgeon General conceded that "[m]ore accurate estimates for the assessment of exposure in the home, workplace, and other environments are needed" (p. 101).

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Separation of Smokers and Nonsmokers

The Surgeon General's Report also concluded that "simple separation of smokers and nonsmokers within the same air space may

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reduce, but does not eliminate, exposure of nonsmokers to environmental tobacco smoke" (p. vii). The Report concludes that smoking bans will not only reduce ETS exposures, but will also "alter smoking behavior and public attitudes about tobacco use" (p. 322). The Report further suggests that "over time, this may contribute to a reduction of smoking in the United States" and provide the basis for the Surgeon General's envisioned "smoke-free society by the year 2000." (p. 322)

The Surgeon General's claim that separation of smokers and nonsmokers does not minimize nonsmoker exposure to ETS is without scientific support. (The claim was made without any reference to the scientific literature.) Scientific studies indicate that the simple separation of smokers and nonsmokers in the workplace, even under conditions of recirculation, can effectively minimize ETS exposure of nonsmokers.[53-64] Moreover, the data suggest that bans and separately ventilated smoking areas do not result in significant reductions of ETS exposures beyond adequate ventilation and/or the simple separation of smokers and nonsmokers under a common ventilation system.

A number of studies in the published literature have evaluated exposure to ETS constituents under various kinds of smoking policies.[53-64] Typical constituents measured include: respirable suspended particulates (RSP and UV-PM), nicotine, carbon

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monoxide and carbon dioxide. The measurements are undertaken in both smoking and nonsmoking areas, usually under general recirculation conditions.

The results reported in the published literature indicate that simple separation of smokers and nonsmokers, under recirculating ventilation conditions, typically reduces nonsmoker exposure by 80% or more.

One recent study reported that the use of designated smoking areas reduced exposure to ETS by as much as 95%.[53] Another study of a smoking-restricted office building reported that ambient nicotine in nonsmoking areas was virtually undetectable, suggesting that ETS had a negligible impact on the nonsmoking areas of the building.[54]

Canadian researchers, in a series of reports, presented results on levels of ETS constituents in offices where smoking was regulated and unregulated. They reported no significant differences in average ETS constituent levels between nonsmoking offices that received recirculated air from designated smoking areas and nonsmoking offices that did not receive recirculated air.[55] Nicotine concentrations reported for nonsmoking areas were marginally above limits of detection; there were no measurable differences in RSP or CO levels in nonsmoking areas that did or

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did not receive recirculated air from smoking areas. They concluded:

> The results indicate that the provision of a designated, but not separately ventilated smoking area can effectively eliminate or drastically reduce most components of environmental tobacco smoke from nonsmoking offices.

Hedge et al., in 1991 reported results of ETS constituent measurements under five different kinds of smoking policies.[56] Their results are reported below:

> A study of the effects of smoking policy on indoor air quality and sick building syndrome symptoms among 3,155 workers in 18 private sector air-conditioned office buildings is described. Five smoking policies were investigated: smoking prohibited [SP], smoking restricted to rooms with local filtration [RF], smoking restricted to rooms with no local air treatment [RNT], smoking restricted to rooms with separate ventilation [RSV], and smoking restricted to the open-plan cubicle work stations and enclosed offices [RWS]. Levels of carbon monoxide, carbon dioxide, respirable particulates, formaldehyde, ultraviolet particulate mass, nicotine, air temperature, and relative humidity were measured at eight sample sites in each building. Approximately 30 workers at each of the eight sample sites completed an extensive questionnaire on environmental conditions, sick building syndrome symptoms, job satisfaction, job stress, smoking history, and personal details. Indoor air quality measures met the current ASHRAE 62-1989 standard. Comparison of all open-office sites between policies showed no significant differences in levels of carbon monoxide, carbon dioxide, formaldehyde or respirable

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particulates. Levels of ultraviolet particulate mass and relative humidity were significantly lower in buildings where smoking is prohibited, and air temperature was significantly higher in these buildings, although on average this was <1°C. Among all buildings, air temperature was the only physical measure that was significantly correlated with SBS symptoms. Smoking policy had no significant effect on sick building syndrome symptoms.

And:

Smoking policy had a relatively small effect on IAQ for the pollutants measured. For most of these pollutants, there were no significant differences in concentration among offices in SP buildings, nonsmoking office areas in RF, RSV, and RNT buildings, and office areas in RWS buildings. There was a significant effect of smoking policy on UVPM and formaldehyde in these office areas, which was due primarily to higher levels in the RF and RSV policies. However, all concentrations of UVPM and formaldehyde were low. UVPM was not significantly correlated with gravimetric RSP, even though the UVPM samples were derived from these RSP samples. UVPM did correlate significantly with metered RSP ($r = 0.69$, $p = 0.0001$) and with nicotine ($r = 0.45$, $p = 0.002$), which suggests that UVPM is measuring particulates from ETS.

Lee and co-workers (1985) evaluated the effects of a smoking policy on airborne ETS constituent levels.[57] They reported:

A new smoking policy was implemented on a trial basis on one floor of a large modern Canadian office building. Smoking was limited to a single enclosed room which shared the same recirculating-type ventilation system with the rest of the floor. Environmental monitoring was conducted on the test floor and a control floor during three consecutive working days both before and after policy implementation.

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Hourly levels of respirable suspended particulate (RSP), carbon monoxide (CO) and carbon dioxide (CO_2) were monitored on the floors and in the designated smoking area throughout the workday. Temperature and relative humidity were monitored at specific sites on each floor and the quantity of outdoor air supplied to each floor was measured on a daily basis. A voluntary questionnaire was circulated to all staff to detect any changes in personal smoking habits over the course of the study.

Results of the investigation showed statistically significant reductions of RSP and CO concentrations on the test floor. After standardization to the control floor, the test floor results indicated a reduction of RSP and CO, 23% and 7.2%, respectively.

In 1989, Proctor and co-workers examined nicotine, respirable particulates, carbon monoxide, carbon dioxide and volatile organics in the air of smokers' and nonsmokers' offices.[58] The data suggested very little nonsmoker exposure to various ETS constituents. The median UV-PM (ultraviolet particulate matter) level in nonsmokers' offices was 8.8 ug/m^3; the median nicotine value was 0.6 ug/m^3. Carbon monoxide and carbon dioxide levels did not differ appreciably between smokers' and nonsmokers' offices. Overall, levels of volatile organics did not differ significantly between smokers' and nonsmokers' offices.

Bayer and Black (1987) reached a similar conclusion in their investigation of volatile organic compounds (VOC) levels in smokers' and nonsmokers' offices.[59] They noted that although

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differences in nicotine concentrations were measured for offices of smokers and nonsmokers, no significant differences in VOC levels were discerned in smokers' vs. nonsmokers' offices. The researchers observed that "it was not possible" to correlate VOC concentrations with ETS or to attribute the source of various VOCs to ETS.

Recent studies on ETS constituent levels aboard commercial aircraft, including a 1989 study performed for the U.S. Department of Transportation, indicate the effectiveness of simple separation of smokers and nonsmokers in the minimization of ETS exposures.[60-62]

Similarly, Proctor (1987) monitored ETS constituents before and after a smoking ban on public transportation in the United Kingdom.[63] While nicotine concentrations decreased from 7 ug/m^3 (micrograms per cubic meter) to 3 ug/m^3 in nonsmoking compartments after the ban, particulate and CO levels remained unchanged. This suggests that ETS contributions to levels of particulates and CO in public transportation are not significant.

Investigators from Healthy Buildings International recently published a paper entitled "The Measurement of Environmental Tobacco Smoke in 585 Office Environments."[64] Using nicotine and particulates as markers for the presence of ETS, the investigators developed a computer model to predict whether a given set of constituent data originated from a smoking or nonsmoking

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area. Using the constituent measurements from a given area, the computer model identified with 96 percent accuracy whether or not the area was "nonsmoking." That is, "spillover" of tobacco smoke was reported in only 4 percent of the areas. As the authors concluded:

> This finding shows that, in general, conventional office ventilation and partitioning is successful in separating smokers from nonsmokers.

Furthermore, the authors concluded:

> Discriminant analysis shows that when 'blindfolded' for presence or absence of smokers, in most cases realistic smoking levels do not significantly influence the aspects of air quality that were measured, and spillover from smoking areas into nonsmoking areas appears to be minimal. This work further reinforces the position the American Society of health Refrigerating and Air Conditioning Engineers (ASHRAE) has taken on ETS in office buildings in ASHRAE Standard 62-89 (1989), in that acceptable air quality can be maintained in properly ventilated offices with a moderate amount of smoking, even without smoker segregation.

The studies reviewed above contain data regarding the low levels of ETS constituents purportedly "transferred" from smoking to nonsmoking areas, even under conditions involving a shared ventilation (recirculation) system. Data reported in those studies indicate that ETS constituents, and particularly nicotine,

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RSP, and CO in nonsmoking areas in buildings where smoking is permitted, are often only slightly above the limits of detection, and often indistinguishable from the "background" levels of constituents that can be found in the air of buildings in which smoking is altogether prohibited. The data support the contention that simple separation of smokers and nonsmokers effectively reduces and minimizes ETS exposure in nonsmoking areas even under conditions of recirculation.

Adult Respiratory Disease

The Surgeon General's Report also addressed purported changes in pulmonary function in adults reportedly exposed to ETS. The Surgeon General concluded that:

> [H]ealthy adults exposed to environmental tobacco smoke may have small changes on pulmonary function testing, but are unlikely to experience clinically significant deficits in pulmonary function as a result of exposure to environmental tobacco smoke alone. (p. 13)

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Two major research groups have examined the possible association between reported exposures to ETS in the workplace and pulmonary function in adult nonsmokers. White and Froeb reported that nonsmokers exposed to tobacco smoke at work for 20 or more

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years had reduced function of the small airways compared to nonsmokers who did not report such exposures.[65]

The White and Froeb study has been criticized for numerous reasons, including its assessment of ETS exposure and its method of subject selection.[66-69] Furthermore, White and Froeb themselves noted that the average values of the pulmonary tests of nonsmokers exposed to tobacco smoke "were not notably different" from the values suggested as normal by a specialist in this area.[70] Dr. Michael Lebowitz of the University of Arizona wrote the following regarding the White and Froeb study:[71]

> Even with a biased population, poor study design, and incorrect statistical evaluation, there were no clear-cut, consistent, medically meaningful differences between passive smokers and groups of nonsmokers; a corrected statistical analysis strengthened this conclusion.

Moreover, the Surgeon General wrote:

> The [White and Froeb] study population was self-selected, and the exposure classification was crude and did not account for people who changed jobs. (p. 60)

In addition to these concerns, the results of White and Froeb appear to be inconsistent with those of a second research group, Kentner, et al., who reported no effect of ETS exposure on

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pulmonary function measurements among 1,351 German office workers.[72] In a 1988 update of the study, Kentner and colleagues noted that, i.e.,

> there is no evidence that average everyday passive smoke exposure in the office or at home leads to an essential reduction of lung function in healthy adults.[73]

The key investigator for the study reported these findings again in 1989 and 1990 publications.[74-75]

The Surgeon General's Report also addressed the issue of acute exposures on the pulmonary function of adult nonsmokers exposed to ETS and concluded that "the magnitude of these changes is quite small, even at moderate to high exposure levels, and it is unlikely that this change in airflow, per se, results in symptoms" (p. 63). The studies available in the scientific literature on asthmatic adults and ETS exposure are clinical studies that have potential applicability to the home and the workplace setting. However, such studies have several problems. The Surgeon General wrote:

> Acute exposure in a chamber may not adequately represent exposure in the general environment. Biases in observation and the [sic] in selection of subjects and the subjects' own expectations may account for the widely divergent results. Studies of large numbers of individuals with measurement of the relevant physiologic and exposure parameters will be necessary to

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adequately address the effects of environmental tobacco smoke exposure on asthmatics. (p. 65)

Cardiovascular Disease

The Surgeon General concluded that further studies "are needed in order to determine whether involuntary smoking increases the risk of cardiovascular disease" (p. 14). In 1988, Fielding and Phenow, reviewers from the University of California-Los Angeles, who are critical of smoking, conceded that "no firm conclusion that a causal relation exists is yet warranted" for reported ETS exposures and cardiovascular disease.[76] A similar view was expressed in a 1988 review from a Harvard physician, who said that there were "no clear data" that ETS increases heart disease risk.[77]

Several additional evaluations of the literature on ETS and heart disease appeared in 1990. Two of these were reported at international conferences in Lisbon, Portugal[78] and in Budapest, Hungary.[79] Both argued strongly that the data on ETS and heart disease were methodologically weak and insufficient to draw conclusions. In another major review, this one from the United States, two physicians, Drs. Mahajan and Huber, concluded that "the data that are available are so sparse that any attempt to reach a definitive assessment would be fraught with uncertainty."[80] Perhaps the most recent review of the literature on ETS and heart

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disease was published in 1991 by A.K. Armitage, the former director of toxicology of a major European research laboratory.[81] He described the scientific data relating to a possible heart disease risk in nonsmokers exposed to ETS as "not very convincing."

In 1986, the Surgeon General's Report concluded that the data regarding possible associations between ETS and cardiovascular diseases and respiratory diseases other than cancer in adult nonsmokers were sparse and inconsistent. Few data have been published subsequent to the Report to warrant any change in that conclusion. Moreover, the Surgeon General's claims that exposure to ETS increases the risk of lung cancer in nonsmokers and that simple separation of smokers and nonsmokers is ineffective in minimizing exposure were not, and currently are not, convincingly supported by the scientific data. The claims thus cannot be used as a basis for workplace smoking policy decisions.

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EPA Draft and Final Risk Assessments

In June, 1990, the United States Environmental Protection Agency (EPA) released for review a Draft Risk Assessment on ETS.[2A] The 1990 Draft Risk Assessment concluded that exposure to ETS is causally related to lung cancer in adult nonsmokers and is associated with respiratory disease and respiratory symptoms in children. The 1990 Draft Risk Assessment also concluded that ETS

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should be classified as a Group A ("known human") carcinogen. It estimated that ETS exposures are responsible for 3,800 nonsmoker lung cancer deaths per year in the U.S. (The estimate was later revised to 3,700.) A committee of the EPA's Science Advisory Board (SAB) reviewed the 1990 Risk Assessment in December, 1990. Their report, presented to the SAB Executive Committee in April, 1991, suggested that while further revisions were needed, the conclusions of the Draft were sound.

In May, 1992, the EPA released a revised Draft Risk Assessment on ETS.[2B] The revised Draft Risk Assessment again concluded that exposure to ETS is causally related to lung cancer in adult nonsmokers, that ETS exposures are causally related to respiratory diseases and symptoms in children, and that ETS should be classified as a Group A ("known human") carcinogen. The revised Draft was reviewed by the EPA's SAB Committee in July, 1992. More charges for revisions were made by the SAB to the EPA staff, but the committee once again endorsed the Draft's conclusions. An Executive Committee meeting of the SAB took place in October, 1992. The Executive Committee endorsed the SAB committee's report, and the Draft was sent back to EPA staff for minor revisions.

On Thursday, January 7, 1993, at a press conference, then-EPA Administrator William Reilly and Secretary Louis Sullivan of the Department of Health and Human Services released the final

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EPA document, entitled Respiratory Health Effects of Passive Smoking: Lung Cancer and Other Disorders.[2C] The document classifies ETS as a Group A ("known human") carcinogen.

All three versions of the EPA Risk Assessment employ a Population-Attributable Risk model for estimating excess lung cancer mortality among nonsmokers reportedly exposed to ETS.[2A,2B,2C] This model is based essentially upon three estimates:

1. a risk estimate for lung cancer derived from a meta-analysis of epidemiologic studies on nonsmoking wives married to smokers;
2. an estimate of the proportion of nonsmokers in the general (U.S.) population reportedly exposed to ETS; and
3. an estimate of the total number of nonsmoker lung cancer deaths in the general population.

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To calculate the Population-Attributable Risk (PAR), the authors of the 1990 Draft Risk Assessment estimated that 60% of all nonsmokers are exposed to ETS. A cumulative relative risk of 1.28 was calculated in the 1990 Draft via meta-analysis of reported results from epidemiologic studies on spousal smoking as the estimated excess risk due to ETS exposure. The PAR for these two assumptions (with other minor adjustments) is 0.27. The total number of deaths due to lung cancer for nonsmoking males and females was also estimated, based on the American Cancer Society's projections for 1988 (9,500 total deaths). By multiplying the PAR

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(0.27) by 9,500, the authors generated an estimate of 2,560 lung cancer deaths per year attributable to ETS exposure among neversmokers in the general population. A PAR was also computed for male and female former smokers, generating a total estimate of 3,800 excess deaths annually purportedly attributable to ETS exposure among nonsmokers in the United States. Using the same procedure in the 1992 Draft, the authors performed a meta-analysis of the results of the eleven U.S. spousal smoking studies then available, and estimated a relative risk of 1.19. The calculated cumulative risk generated an estimate of approximately 3,000 excess deaths per year among nonsmokers. The EPA did not modify the estimate presented in the 1992 Draft in the final risk assessment.

The PAR method employs estimates of relative risk, population fractions of estimated exposure to ETS, and lung cancer death rates for the general nonsmoking population in order to generate an estimate of excess mortality reportedly attributable to ETS exposure. It is important to note at the outset that the PAR model itself does not determine that there is an increased risk of lung cancer among nonsmokers from ETS exposure. Rather, the model assumes a causal relationship between ETS exposure and an increased risk of lung cancer among nonsmokers, based upon increased risks reported in epidemiologic studies on spousal smoking. These reported relative risks are, in turn, assumed to

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represent true relative risks for the entire population due to ETS exposure.

These critical assumptions have been challenged.[82] To achieve a cumulative excess risk estimate of 1.28 for nonsmokers reportedly exposed to ETS, the authors of the 1990 Draft Risk Assessment performed a meta-analysis of 23 epidemiologic studies on spousal smoking. Yet, eighteen of the studies on spousal smoking included in the EPA's 1990 meta-analysis reported overall risk estimates that failed to achieve statistical significance and were, therefore, consistent with the null hypothesis of no association between spousal smoking and lung cancer among nonsmokers.

The meta-analysis in the 1992 Draft was performed on eleven U.S. spousal smoking studies, none of which originally reported a statistically significant overall risk estimate. Nevertheless, the EPA combined the results of the studies and generated a summary risk estimate of 1.19 for nonsmoker lung cancer. The EPA reported that its risk estimate, obtained by reanalyzing the data from the original studies and applying a one-tailed test for statistical significance (with its accompanying 90% confidence interval), was statistically significant. The one-tailed test, however, presumes causation: it is designed to show the extent of a purported "effect." A two-tailed test at 95% confidence

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intervals would have generated a statistically nonsignificant summary risk estimate for the U.S. studies.

The choice of statistical test and confidence interval was criticized in a presentation by James J. Tozzi at the October SAB Executive Committee meeting. In a follow-up letter to the EPA in December of 1992, Tozzi included a meta-analysis of the 13 then-available U.S. studies. The summary risk estimate is not statistically significant using a standard 95% confidence interval.[83] Tozzi reported a relative risk estimate of 1.07 (95% C.I.: 0.95 to 1.21). The EPA did not choose to modify their final risk estimate in light of the new data.

The meta-analysis submitted by Tozzi included data from two additional lung cancer studies that reported statistically nonsignificant overall associations between spousal smoking and lung cancer. The EPA omitted from its risk assessment the data from the Brownson, et al. (1992), study, one of the largest and most recent studies on ETS and lung cancer, which reported no increase in overall risk for spousal smoking.[22] The Brownson, et al., study was partially funded by the National Cancer Institute (NCI). EPA also omitted data from the Stockwell, et al. (1992), study.[23] If the EPA had considered the Brownson and Stockwell data, its meta-analysis would not have calculated a statistically significant

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increased risk estimate for spousal smoking, at either a 90% or 95% confidence interval.

Eleven of the epidemiological studies EPA considered in the 1992 Draft Risk Assessment also included estimates of workplace exposures to ETS and lung cancer in nonsmokers, but the EPA did not assess the data from those studies. Ten of the eleven studies reported no statistically significant increased risk for nonsmoking females. If the data on workplace exposures are pooled in a meta-analysis, the risk estimate approximates 1.00 (unity), which indicates no positive association between reported workplace exposures to ETS and lung cancer in nonsmokers.[84-85]

The epidemiologic studies on spousal smoking contain no actual exposure measurement data on ETS. The 1990 and 1992 EPA Drafts assumed the validity of questionnaire responses about possible exposure to ETS based upon spousal smoking and then generalized those responses to estimate the general population's exposure to ETS. The EPA did not address the important issue of recall bias for exposure estimation or the problems in extrapolation to the workplace environment. The authors of the 1990 and 1992 Draft Risk Assessments also failed to adequately consider and adjust for possible confounding factors, e.g., diet, lifestyle, genetics, etc., the data for which are available in the scientific literature. This is a significant oversight, especially when dealing with "weak"

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relative risks which approximate 1.3 in the 1990 Draft Risk Assessment and only 1.19 in the 1992 Draft.

Other conclusions of the 1990 Draft Risk Assessment were strongly criticized in many of the more than 100 comments submitted during the public comment period on the draft.[82] Specifically, many of the public comments found EPA's classification of ETS as a Group A carcinogen to be scientifically unwarranted.

One point of criticism was that the EPA's proposed classification of tobacco smoke as a "human carcinogen" was based in part upon the imputed identification and presence in ETS of suspected carcinogens reported in mainstream smoke and/or fresh sidestream smoke. However, the EPA did not review the available published data on either the characterization of, or exposure to, ETS in its hazard identification. ETS is neither chemically nor physically equivalent to either mainstream or sidestream smoke, and it is therefore not scientifically acceptable to treat ETS, mainstream smoke and sidestream smoke as physically and quantitatively similar mixtures.[86-91]

As discussed above, meta-analysis, a statistical procedure which combines the reported risk estimates from a number of studies to generate an overall estimate of risk, was used in the EPA's Population-Attributable Risk model. The problems and limitations

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of the use of meta-analysis for the epidemiologic studies on spousal smoking have been addressed in the scientific literature.[92,93] For instance, meta-analysis does not account for intrinsic bias or confounding in the individual studies, and it cannot remedy flaws in study design. As two German scientists, Heinz Letzel and Karl Uberla, noted: "Combining risk estimates from biased or confounded studies by meta-analysis cannot provide correct answers."[93]

Another major criticism of the 1990 Draft Risk Assessment was that in addition to its inadequate treatment of the data on the physical and chemical properties of ETS, it also virtually ignored available exposure data, toxicologic data and data from animal inhalation studies on ETS. The 1992 Draft similarly failed to consider these data. A large body of literature exists on actual measurements of ETS in indoor environments, which was not considered in the 1990 Draft Risk Assessment and only cursorily considered in the 1992 Draft.[94-104] While these data are independent of the epidemiologic literature -- not one of the spousal smoking studies included actual measurements of ETS exposure -- they suggest that nonsmoker exposure to ETS in typical public places and workplaces is minimal.[94-97] For example, some studies report typical measurements of nicotine ranging from an exposure equivalent of 1/100 to less than 1/1,000 of one filter cigarette per hour.[98-104]

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While a public docket for written comments was not established for the 1992 Risk Assessment, comments were accepted at both the July 1992 SAB committee and the October 1992 Executive Committee meetings. One major issue raised by Dr. William J. Butler in July was the EPA's treatment of confounders.[105] The EPA took the position that confounding could not account for the reported association between spousal smoking and nonsmoker lung cancer, since the EPA was unable to find (or "think of") a single confounding factor that could consistently account for the results reported in all of the studies used in the Draft Risk Assessment. Butler criticized the EPA's position and offered data to indicate that potential confounders are contingent upon the selection and matching of cases and controls in each study, and that it was not necessary (or even reasonable to expect) to identify a single confounder that would apply to all of the individual studies.

Had the EPA followed its own 1986 guidelines for carcinogen risk assessment, it would have included: (1) a hazard evaluation, which would have examined data regarding the physical and chemical characterization of ETS, as well as the results from published animal inhalation studies and human short-term tests; (2) an exposure evaluation, which would have included and integrated the data from well over 35 studies in the published literature that monitored ETS constituents in the air of homes, public places and workplaces; (3) a dose-response evaluation, which would have

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relied upon the actual data and trends reported in the epidemiologic studies on spousal smoking (and not upon "modified" trend tests); and (4) a risk characterization, which would have included the range of uncertainty in assumptions for the numbers of lung cancer deaths supposedly attributable to ETS exposures. The guidelines also require that chance be ruled out statistically and that all possible biases and confounding factors be considered if epidemiologic studies are relied upon. The EPA failed to satisfy any of those criteria. The EPA therefore could not have classified ETS as a Group A carcinogen had it applied the methodologies and guidelines it used in its risk assessments for other substances.

Dose-Response Issues

In an apparent attempt to buttress its position on the epidemiology of spousal smoking, the EPA has recently presented a series of arguments related to ETS dose-response issues. During a July, 1993 hearing before a House Subcommittee on Agriculture, Dr. William H. Farland, Director of EPA's Office of Health and Environmental Assessment of the Office of Research and Development, stated:[106]

> Using the crude 'exposed' versus 'unexposed' measure, 24 of the 30 [spousal smoking] studies found an increased risk of lung cancer in the exposed group; 9 of these were statistically significant. This proportion (9/30) of significant studies is highly unlikely to have

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occurred by chance (probability less than 1 in 10,000). (p. 9)

The alleged statistical significance for nine of the 30 spousal smoking studies was generated by the EPA's reanalysis of the study data and the subsequent expansion of confidence intervals originally reported for the 30 studies. Only six of the 30 studies originally reported a statistically significant increased overall risk for nonsmoking women married to smokers. All six studies were conducted outside the United States. Two of the studies were "hypothesis-generating" (Hirayama and Trichopoulos, et al.), and both were conducted in 1981. Three of the four other studies claiming statistically significant results were conducted on Oriental women, in which critical confounding factors such as cooking and heating practices were not adequately controlled. No subsequent study with statistical power equal to or greater than the Hirayama or Trichopoulos studies has reported an overall statistically significant increased risk for spousal smoking. Thus, 24 of 30 studies (80%) examined by the EPA reported results that are compatible with the null hypothesis of no association between spousal smoking and an increased risk of lung cancer in nonsmoking women.

A second argument offered by Dr. Farland was as follows:[106]

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> All 17 studies with data categorized by exposure level (i.e., amount of spousal smoking) found an increased risk of lung cancer in the highest exposure group, and 9 of the 17 were statistically significant (probability less than 1 in 10,000,000), despite most having a small sample size. (p. 9)

Earlier in his testimony, it was conceded that the spousal smoking exposure index, used as a proxy or a surrogate for actual exposure data on ETS constituents, "is a crude measure and the studies are prone to exposure misclassification." (p. 9) The accuracy (or inaccuracy) of the spousal smoking index as a quantitative indicator of ETS exposure is the heart of the dose-response issue. Studies comparing actual exposure data for ETS constituents in the ambient air with responses to questionnaires about ETS exposure indicate extremely poor agreement between the two indices.[45-47,107-109] Comparisons of the categories "exposed" versus "unexposed" wives cannot be precisely related to ETS exposure. Rather, they serve as indicators for the broad category of "marriage to a smoker," with all the concomitant and potential confounding factors that are involved. In essence, the EPA has no actual exposure data upon which to rely for their dose-response analysis.

In the current argument, the EPA claims to have examined the data from 17 studies categorized by "exposure" level, although the 17 studies are not identified. The "data" from the

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questionnaire response categories on exposure in the 30 spousal smoking studies, however, exhibit no uniformity and are of questionable comparability. For example, the category "highest level of exposure" is defined differently across studies, sometimes in terms of "years of exposure," sometimes in terms of "cigarettes per day;" in other studies, it is defined in terms of "hours per day" or "pack years of exposure." Moreover, within those studies that employ the exposure category "cigarettes per day," no uniform level for determination of the "high" level of exposure is presented from one study to the next. For example, one study uses "20+ cigarettes" for its designation of high exposure, another uses "26+ cigarettes," another "16+ cigarettes," and still another, "30+ cigarettes." There is simply no comparability of "highest exposure level" category across studies.

The EPA nevertheless concludes that, of the 17 studies amenable to a "highest exposure" determination, nine reported a statistically significant increased risk of lung cancer. Although details of its analysis are not offered, the EPA has apparently compared the base risk for "unexposed" (RR = 1.0) individuals with the risk reported for "highest level" of exposure in each study. Such statistical comparisons are dubious because they depend upon unreliable dose markers and estimates.

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The EPA's third argument is related to the second. Farland states:[106]

> . . . Ten of the fourteen studies with sufficient data for a trend test showed a statistically significant exposure-response relationship (probability less than 1 in 10 billion), i.e., increasing risk of lung cancer with increasing ETS exposure. (p. 10).

Contrary to the EPA's claim, one would be hard-pressed to find ten of the original 30 spousal smoking studies which offered consistent, much less statistically significant, trends for dose-response. Again, the so-called trend test "results" offered by the EPA are derived from their reanalysis of the data from the original spousal smoking studies. Unfortunately, as has been pointed out in several comments from statisticians, the EPA performed improper trend tests on the data.[110,111] The authors of the Risk Assessment recalculated trends for dose-response by adding what statisticians call a "pseudo-datum" representing zero exposure. The addition of the "pseudo-datum" converts statistically nonsignificant dose-response trends, as originally reported in the individual studies on spousal smoking, into "statistically significant" trends. The use of this inappropriate procedure would essentially permit the establishment of a so-called dose-response trend based upon any single category of exposure greater than zero.

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A test for overall significance of risk compares the risk at zero exposure with the risk in a group of exposed subjects. Here, statistical significance simply establishes that chance alone did not account for the difference between "unexposed" and "exposed" groups. On the other hand, for the assessment of a dose-response relationship, one compares the reported risks from groups who report exposure at different levels, all of which are positive.[109] Here, the "unexposed" or zero exposure group contributes nothing to the dose-response assessment. Thus, the EPA is simply mistaken in its claim that the data from the studies on spousal smoking demonstrate statistically significant trends for dose-response. If the statistical analysis of the dose-response data had been performed correctly, EPA would have no foundation for its claim.

NIOSH Current Intelligence Bulletin

In June 1991, the National Institute for Occupational Safety and Health released Current Intelligence Bulletin 54 (NIOSH CIB) on environmental tobacco smoke.[3] The NIOSH CIB concluded that:

> NIOSH has determined that the collective weight of evidence (i.e., that from the Surgeon General's reports, the similarities in composition of MS and ETS, and the recent epidemiologic studies) is sufficient to conclude that ETS poses an increased risk of lung cancer and possibly heart disease to occupationally exposed workers. (p. 12)

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The NIOSH CIB refers to three areas that contribute to its "collective weight of evidence" assessment. The Surgeon General's report is the first area; however, the conclusions of the Surgeon General's report have been extensively criticized, as discussed above. The second type of "evidence" (the claimed "similarities in composition of MS and ETS") proffered by the NIOSH CIB is neither detailed nor complete.

Although both the NIOSH CIB and the EPA Risk Assessment stress the reported association between active smoking and disease in reaching their conclusions about ETS, neither report provides detailed comparisons of the chemical and physical natures of, and differences between, ETS and mainstream smoke (MS). ETS is physically different from both mainstream and sidestream smoke, and its chemical properties are distinct from either mainstream or sidestream smoke.[86-91]

One major area overlooked by the NIOSH CIB is the scientific studies that actually measured levels of ETS constituents in indoor air in the workplace. There is a substantial body of literature in this area which is directly relevant to considerations about non-industrial workplace exposure to ETS constituents.[55-56,58-59,94-96,98-103] If the data on actual ETS exposures in the workplace are examined, one finds that typical

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workplace exposures to ETS are minimal and often below limits of detection for even the most sensitive tobacco smoke constituent monitors.

The third element of the NIOSH CIB's "weight of the evidence" argument is "recent epidemiologic studies" on ETS and lung cancer. However, its review of the spousal smoking studies is incomplete. The NIOSH CIB states that eight additional spousal smoking studies had been published since 1986, when, actually, 14 had been published (through 1991), most of which report overall associations that do not achieve statistical significance. Furthermore, the NIOSH CIB acknowledges serious shortcomings in the available epidemiologic studies purporting to relate ETS exposure and lung cancer:

> NIOSH recognizes that these recent epidemiologic studies have several shortcomings: lack of objective measures for characterizing and quantifying exposures, failure to adjust for all confounding variables, potential misclassification of ex-smokers as nonsmokers, unavailability of comparison groups that have not been exposed to ETS, and low statistical power.

Nevertheless, the NIOSH CIB uses spousal smoking studies to reach its conclusion about occupational exposure to ETS without justifying the relevance of spousal smoking studies to workplace exposure. The NIOSH CIB also fails to directly address the 10

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then-available spousal smoking studies that included specific questions about workplace exposure.[9-15,17-21] Eight studies reported no statistically significant increased risk of lung cancer for nonsmokers who reported ETS exposure in the workplace. The other two risk estimates are of borderline statistical significance. When the data on reported workplace exposures to ETS are combined, meta-analysis yields a risk estimate that is indistinguishable from unity.[84-85] The epidemiologic data on workplace exposure to ETS are consistent with the null hypothesis, i.e., that there is no association between workplace ETS exposure and nonsmoker lung cancer.

The NIOSH CIB also purports to review the studies on ETS and heart disease that have appeared since 1986. While additional studies have been published since 1986, taken as a whole, the studies still do not provide sufficient data for a conclusion regarding heart disease in nonsmokers and workplace exposure to ETS.[112] Moreover, the epidemiologic studies on ETS and heart disease can be criticized for employing spousal smoking as a surrogate for ETS exposure and for failure to adequately account for confounding factors. The latter include: family history of heart disease, blood pressure, serum lipoprotein levels, body mass index, age, menopausal status, dietary fat intake, alcohol consumption, diabetes and lifestyle factors (e.g., a sedentary lifestyle).

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Thus, the NIOSH CIB, which reaches conclusions about purported risks associated with exposure to ETS in the workplace, is not a comprehensive, critical review of the available data on ETS. Of even greater significance, it does not address available workplace data on either ETS exposures or claimed health effects.

Other Risk Assessments on ETS

Two basic methods (modeling procedures) for estimating the population risk for lung cancer among nonsmokers reporting exposure to ETS have appeared in the scientific literature. The first method, adopted by the U.S. EPA in its Risk Assessment (discussed above), is the Population-Attributable Risk (PAR) approach. A PAR model seeks to establish an estimate of excess risk ostensibly due to ETS exposure and is expressed as a ratio of the risk assessed for ETS to the total lung cancer risk for nonsmokers from all sources. The PAR is calculated by applying a relative risk estimate associated with reported exposure to ETS (derived from risk rates in epidemiologic studies) to the percentage of individuals believed to be exposed to ETS in the general population.

A second approach used in the literature, called the Extrapolation Method (also called the Linear Extrapolation Method or Dose-Response Extrapolation Model), combines reported risks of

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lung cancer for active smokers, derived from epidemiologic studies on smokers, with estimates of tobacco smoke exposure (dose) for active smokers. The risk and dose estimates for active smokers are then extrapolated downward to apply to nonsmokers. The estimated excess risk for nonsmokers exposed to ETS is obtained by dividing the lung cancer risk reported for active smokers by the ratio between the smokers' and nonsmokers' estimated average exposure to tobacco smoke.

This second approach for estimating excess lung cancer deaths among nonsmokers reportedly due to ETS exposure is exemplified by the model developed by Repace and Lowrey (1985).[113] The authors employed a linear downward extrapolation from the lung cancer risk reported for active smokers (and estimates of tobacco smoke exposure for smokers) to an exposure and residual risk estimate for nonsmokers allegedly exposed to ETS. The model estimated lung cancer mortality among nonsmokers by dividing the reported lung cancer risk for active smokers by a ratio of estimated tobacco smoke exposure for smokers and nonsmokers. The linear dose-extrapolation model therefore requires four estimates:

1. the number of nonsmokers supposedly exposed to ETS;
2. the average ETS exposure of nonsmokers;
3. the average tobacco smoke exposure for active smokers; and

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4. the lung cancer risks reported for current active smokers.

Specifically, Repace and Lowrey developed a version of a linear dose-response extrapolation model which is based upon a weighted average of nonsmoker exposure to particulates. The weighted average estimate was not derived from actual exposure data but from a model which predicts ambient concentrations of particulate matter from the number of cigarettes smoked in a given volume of air space. The authors also calculated a lung cancer risk estimate and an estimate of daily "tar" (particulate) intake for active smokers. Nonsmoker risk of lung cancer was extrapolated from those estimates to yield 555 lung cancer deaths per year attributable to ETS exposure among nonsmokers.

Arundel, et al., (1987) refined the Repace and Lowrey extrapolation model by replacing Repace and Lowrey's estimates of particulate matter exposure for nonsmokers with actual exposure data from monitoring studies.[114] The Arundel, et al., model also rejected Repace and Lowrey's extrapolation from the dose of active smokers to the exposure of nonsmokers, and replaces the latter with an estimated retained dose of particulates for nonsmokers. Using virtually the same assumptions as Repace and Lowrey, the Arundel et al. model estimates 12 lung cancer deaths per year among 40 million male and female neversmokers.

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The various assumptions and estimates employed in the Repace and Lowrey linear dose-extrapolation model have been challenged by a number of scientists.[114-118] One scientist noted that the exposure and dose levels Repace and Lowrey used were not based on actual measurements, and that actual measurements reported by other researchers ranged from "ten-to-one-hundred-fold less than that in the Repace and Lowrey model."[115]

Still other scientists have questioned the methods of analysis used in their article.[116-118] For example, the Repace and Lowrey extrapolation model assumes that the "carcinogenicity" of tobacco smoke depends upon some (unknown) element purportedly located in the particulate phase of ETS. The model also assumes that lung cancer per unit of exposure (i.e., per mg of "tar") is the same for mainstream smoke and ETS, an assumption which is not borne out by the scientific data regarding the chemistry of mainstream smoke and ETS. Moreover, the extrapolation model suggests that the relationship between reported risk and level of exposure is linear (i.e., dose-response), and it assumes that there is no exposure level below which lung cancer risk is absent. The dose-extrapolation model also assumes, with its suggestion of a linear dose-response from active smoking to low level exposure to ETS, that the so-called "one-hit" cancer theory, a theory that one molecule of exposure to a suspected carcinogen is sufficient to induce carcinogenesis, is valid. However, the "one-hit" model has

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what statisticians call a "zero intercept term," which is equivalent to assuming that lung cancer risk among nonsmokers is zero in the absence of ETS.

Even with its dubious assumptions and estimates, it is noteworthy that the linear dose-extrapolation model produces estimates of excess lung cancer deaths which are roughly an order of magnitude lower than estimates generated by the PAR model which relies on epidemiologic studies. A number of subsequently published extrapolation models, unlike that of Repace and Lowrey, have been based upon actual data and reasonable estimates of exposure.[43,114,119-120] These models have been unsuccessful in estimating any appreciable increased risk of lung cancer for nonsmokers reporting exposure to ETS. Indeed, extrapolation models based on estimates of retained particulate matter generate estimates of excess risk which are orders of magnitude lower than estimates generated by the PAR method. Such vast ranges underscore the uncertainties and dependencies upon assumptions for both risk estimates based on epidemiology and those based on dosimetry. The differences in risk estimates are so striking that even the authors of the EPA Draft Risk Assessment were unable to generate a "dose-response based on the extrapolation from mainstream to environmental tobacco smoke," and hence, relied solely upon the PAR model to support their contention of an increased risk of lung cancer among nonsmokers reportedly exposed to ETS.[121]

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There have been two published risk assessments of environmental tobacco smoke (ETS) exposure and heart disease, the first by A. Judson Wells in 1988,[122] and the second by Kyle Steenland in 1992.[123] However, despite his never having published a risk assessment on ETS and heart disease, Stanton Glantz is the individual most often cited for the conclusion that a large number of annual heart disease deaths in the U.S. are attributable to ETS exposure. In a 1991 article with William Parmley,[124] Glantz claimed that the number of annual ETS-related nonsmoker deaths in the U.S. is approximately 53,000, the largest portion of which (37,000) he claimed was from heart disease.

The figure of 53,000 annual U.S. deaths attributed to ETS exposure originated in a publication by A. Judson Wells in 1988,[123] although Wells actually argued for a "best estimate" of ETS related deaths that was somewhat lower, about 46,000, with 32,000 of these due to heart disease. Stanton Glantz and William Parmley repeated the 53,000 figure in their 1991 article in Circulation. However, they did not themselves independently perform a calculation of "excess" deaths, but instead relied on the earlier work by Wells, which is cited by Glantz and Parmley as the basis for their claim.

To calculate excess deaths due to ETS exposure, Wells used meta-analysis to derive ETS-associated relative risks for lung

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cancer, heart disease and cancers other than the lung. These relative risks, together with an estimate of the fraction of the population exposed to ETS, were entered into an equation that was used to calculate excess death rates for never smokers. To derive a value of actual numbers of deaths related to ETS exposure, these excess death rates were multiplied by the number of people in the U.S. estimated to be exposed. Based on such calculations, Wells estimated that ETS exposure produced 39,000 excess deaths per year in the U.S.

Further calculations attempted to take into account misclassification, either of smokers as nonsmokers or of nonexposed nonsmokers as exposed to ETS. Wells calculated that these adjustments for misclassification resulted in a revision upward of the estimate, from 39,000 to 53,000. Wells then stated that a more "conservative" estimate would be 46,000, a value chosen because it is halfway between the unadjusted value of 39,000 and the adjusted value of 53,000.

It is clear that the claim of 53,000 excess deaths associated with ETS exposure is assailable at every level of its derivation. At the most fundamental level, the studies contributing to the meta-analysis are scientifically flawed. The flaws stem from, among other reasons, small sample sizes, inadequate control for potential confounding factors and unreliable estimates of ETS

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exposure. Such flaws contribute to the invalidity of the subsequent meta-analysis, a technique which is further called into question because it is not designed to bring together such methodologically disparate studies. Furthermore, any overall estimate of the prevalence and levels of ETS exposure within the U.S. is based on assumptions, rather than direct measurement. In addition, a variety of other unproven assumptions are made in attempting to adjust the data to take potential misclassification into account. Finally, the seemingly capricious derivation of the annual death rate claim is reflected in Wells' final calculation in which he decided, based on no particular formula, that the value of 46,000 would be a "best estimate," simply because it was halfway between an unadjusted and adjusted estimate. Such loose and unscientific conclusions should not form a basis for regulatory action.

Kyle Steenland, a National Institute for Occupational Safety and Health employee, also performed a risk assessment of ETS and heart disease, published in 1992.[124] He calculated that 35,000-40,000 annual U.S. heart disease deaths are attributable to ETS exposure. He concluded that "heart disease mortality is contributing the bulk of the public health burden imposed by passive smoking."

There are two important differences between Steenland's estimation process and that used by Wells. First, Steenland did

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not do a meta-analysis to obtain a pooled estimate of relative risk for heart disease mortality associated with ETS exposure. Instead, he simply adopted the relative risk reported in a single study of a Maryland sample[125] and applied it to the entire U.S. population. Second, he focused only on heart disease and did not attempt to calculate purported ETS-related risks from other diseases.

Other than the above, Steenland's procedure for calculating deaths attributable to ETS exposure was generally similar to that reported by Wells. This estimation process involved: positing an overall increase in relative risk of heart disease associated with ETS exposure; making adjustments for potential misclassification and for background exposure; estimating the extent of exposure to ETS; and estimating the fraction of nonsmoker heart disease deaths attributable to ETS exposure. These estimates were incorporated into a formula using data on U.S. heart disease death rates and population estimates, from which was derived an estimated number of annual heart disease deaths attributed to ETS exposure. According to Steenland's calculations, "the overall estimate of ETS-attributable heart disease deaths for never-smokers and former smokers is 35000 to 40000." He further commented that these increased risks of death "are higher than those accepted in regulating environmental toxins."

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Steenland's calculations involved problems similar to those that plagued Wells' attempt to derive a number of deaths associated with ETS exposure. Although Steenland's approach did not use meta-analysis, his procedure can perhaps be criticized even more severely because he relied upon a relative risk based on a study from a single county and applied it to the United States as a whole. Criticisms relating to unverified assumptions, which were noted in regard to Wells' calculations, also apply to Steenland's procedure.

Summary

The conclusions of governmental reports and other publications on ETS are relied upon extensively by those who claim an association between ETS exposure in the workplace and adverse health effects in nonsmokers. However, these reports and assertions have serious deficiencies that render their conclusions unsupported. When appropriate scientific scrutiny is exercised, the reports and articles do not provide a scientifically defensible basis for the regulation of smoking in the workplace.

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